Author's response to reviews

**Title:** Social Relationship Correlates of Major Depressive Disorder and Depressive Symptoms in Switzerland: Nationally Representative Cross Sectional Study

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**Author's response to reviews:** see over
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Dear BMC Public Health:

Attached please find a revised manuscript titled “Social Relationship Correlates of Major Depressive Disorder and Depressive Symptoms in Switzerland: Nationally Representative Cross Sectional Study.” We are grateful for the opportunity to revise the manuscript and for the specific, constructive referee feedback.

Our responses to the reviewer’s feedback are below in Courier typeface.

Reviewer’s report:
The authors address an important question of the relative contribution of various social relationship factors to depressive symptoms and major depressive disorder in a very large sample of Swiss adults. The article is well-written and concise. I have just a few comments (below):

Thank you for the positive feedback.

Minor Essential Revisions
1. The first sentence of the ‘Methods’ section of the Abstract is not a complete sentence.

We reworded the first sentence of the Methods section in the Abstract.

2. On p. 4, the authors are missing the word ‘of’ in the following sentence: “have used a broad, theoretically representative set of relationship domains” and missing the word ‘a’ in the following sentence: “which are each comprised of a number of dimensions.”

We made the suggested corrections on p. 4.

3. On p. 6, the authors need to make ‘utilize’ past tense (i.e., “We utilized data...”)

We corrected the verb tense for “utilized.”

4. It is not clear what all of the numbers represent in Table 3. The authors need to state in the title of the table what the various numbers (e.g., in parantheses) represent.

The numbers in Table 3 represent incidence rate ratios and their 95% confidence intervals (in parentheses). These are described as such in the Table 3 title so we are unclear as to what additional labeling is required. We did add a note to Table 3 describing the abbreviation “CI.” We also noticed that the abbreviations describing parentheses content was
missing from Table 1 so we added those to the Table 1 legend.

Major Compulsory Revisions

5. The description of the social relationship measures is confusing. First, it is not clear whether the items provided represent example items or are indeed single items (e.g., to measure ‘unmet support needs’, is the question “Do you ever miss having someone to talk to about your problems” the only item that assesses this?) Furthermore, the authors write that social contacts were defined using the sum of 5 questions, but it is not clear what those questions are. It is also unclear whether “social contacts” is one of these 5 questions or a separate variable. Also, what do the authors mean by “social contacts were coded in four indicator variables representing 5 categories” – and do those 5 categories represent number of people? (e.g., in the 0-9 category represents having 0 to 9 social contacts?)

We have more carefully described the social relationship assessments. We specified how many items were available for each construct and provided the full item and response options in the text body. We also clarified how the social contact indicator variables were constructed, i.e., the ordinal responses were summed and collapsed into 5 categories. We hope that these revisions make the methods clearer.

6. Limitations/Issues – 0 is very different from 1, which is very different from more than one (problematic that 0 to 9 is combined)

We evaluated the social contact operational definition in light of the reviewer’s suggestion that zero contacts is different from 1 contact which is different from >1 contacts. Comparing zero to 1 in the summary measures showed that those scoring zero had more depressive symptoms (means of 6.7 vs. 4.3) and a higher prevalence of 12-month major depressive disorder (27.5% vs. 0.0%). However, these groups had very small cell sizes (n’s of 14 and 6, respectively) and both point estimates for the 1 contact group were within the 95% confidence intervals for the zero contacts group. In fact, for major depressive disorder the 95% confidence interval for the zero contacts group (0.007-0.545) included all of the point estimates for the remaining 21 social contact categories. A very similar pattern was observed for depressive symptoms – the confidence interval for zero contacts encompassed 20 of 21 social contact categories point estimates. These analyses show: 1) that there would be substantial imprecision in point estimates when comparing depression measures across the lower range of social contacts (i.e., the 0-9 range) and 2) that aggregating these categories is desirable because aggregation provides more stable estimates and does not mask reliable depression differences across these lower categories. We are grateful
to the reviewer for raising this issue and we hope that these analyses address those concerns. We added a sentence about small cell sizes at this range of the social contact summary measure in the revised manuscript. We also noted studies that similarly aggregated social contact frequency items.

7. On p. 9 the authors write that confidant and tangible support were ‘scaled’ – how was that done? And how does that make these variables comparable to loneliness and social contacts, which were measured on continuous scales?

We regret that the coding for the confidant and tangible variables support was unclear. We did this coding to address scale dependence of regression coefficients (e.g., Cohen, Cohen, West & Aiken 2003 [3rd Ed.] Applied multiple regression/correlation analyses for the behavioral sciences, p. 511). For example all things being equal, rate ratios will be larger for a binary variable versus a variable with >2 categories. We wanted to make these 3-category items (confidant and tangible support) range from 0 to 1 to match other binary predictors (e.g., marital status, unmet support). It is correct that social contacts and loneliness have more than three categories and that these were not rescaled. Loneliness did not meet an interval assumption and therefore had to be coded using indicator variables, i.e., the rate ratios were of different magnitude across levels of loneliness.

The social contacts variable did meet the interval assumption but because we used 5 categories we thought it easier for the reader to see the indicator variables in the regression analyses. These categories also parallel other operational definitions of social contacts and thus facilitate comparison with prior research (cf. reference 52 (CHECK-Barger 2013?)

These scaling changes mean that each coefficient in Table 3 represents an identical unit range for all social relationship measures. We clarified this in the text body and in the legend to Table 3. We are attempting to utilize optimal analytic practices and we hope that these choices and their rationale are clearer now.

8. The four education categories should be specified in the text.

We specified the four education levels in the text.

9. On p. 9 the authors write that three social relationship variable had more than two categories (loneliness, confidant, and tangible support) – but what about social contacts? That also had more than two categories.

As noted by the reviewer, social contacts had more than two categories. We preserved the 5-category analysis to provide
comparability with other studies examining social contacts. The ordinal scaling of social contact frequency is common in the literature and this metric will facilitate comparison with prior research (e.g., Am J Epidemiol 2012;175(12):1275-1283; Ann Behav Med 2003;26:15-23.).

10. I am curious as to whether the researchers have considered a possible confound between loneliness and depressive symptoms. I am not familiar with the DSQ, but the CES-D for example has items that essentially assess loneliness, making it difficult to correlate loneliness with depressive symptoms. At the very least, this should be acknowledged as a limitation.

Confounding of loneliness and depression is a very important issue. Other depressive symptom assessments such as the CES-D have loneliness items and thus associations could be contaminated by predictor-criterion overlap. Some studies using the CES-D omit this item (e.g., Cacioppo et al., 2010) whereas others do not (George et al., 1989). However, the DSQ (the depressive symptom assessment used in this study) has no loneliness items or any other social relationship items for that matter. Thus predictor-criterion overlap is not an issue for this assessment and we added a sentence to this effect in the Methods section. We added a description of all 10 DSQ items to make this independence clearer. We also cited the CES-D as an example of a depression questionnaire that contains loneliness content. We thank the reviewer for highlighting this important issue.

Given the long lag between our submission and the editorial decision we conducted another literature search that revealed several related studies published in the last year. We integrated those studies in the first paragraph of page 14. Our last revision was to add a sentence noting that this analysis of archival data was exempted from IRB review by Northern Arizona University.

The lead author of this paper (Steven Barger) is a Section Editor for BMC Psychology so we would like for that editorial discount to be reflected should the time come to pay the article processing charges.

Thank you for your consideration. We look forward to hearing from BMC Public Health.

Sincerely,

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